and in staining them in animal tissues, are described in a lucid though rather a dogmatic fashion. The *rôle* and the nature of the *rôle* the pathogenic bacteria play in relation to infectious diseases are, as the title indicates, the principal subjects of the book, and we have no hesitation in saying that the authors have produced a very creditable work. But there are a number of diseases of the lower animals described here, in which the demonstration of bacteria in microscopic sections through one or the other of the diseased organs is sufficient for the authors to assign to those bacteria a specific character; more than this, every granule which appears stained in those sections is regarded by the authors as a microbe. The exact proof, and even attempt at exact proof, that this is so is omitted in many of these cases.

The illustrations accompanying the text are very numerous, many of them in colours: amongst these latter some are very excellent and true to nature, *e.g.* those illustrating the chapter on tuberculosis; many others are decidedly bad and erroneous, *e.g.* most of the illustrations of cultivations, and some of the sections stained in one or two colours, while many others are badly printed. We fail to see the use of such a confusing and diagrammatic crowd of illustrations as form plate i. The authors give numerous references to other workers, but there is, we think, a rather large dose of reference to their own works, such reference occurring in remarkable sameness. In several instances they do not seem to have read the original statements of their references. Thus, for instance, Cornil and Chantemesse in 1887 described and illustrated the pathology of the disease known in England as swine fever; they describe it under the name of "pneumo-entérite des porcs," both the name and the pathology of which disease have been minutely described and copiously illustrated in the Reports of the Medical Office of the Local Government Board for 1878–79, under the name of "infectious pneumo-enteritis of the pig." If they had really read the text in that English Blue-book, and inspected the illustrations of the pathology of our infectious pneumo-enteritis of the pig, they could not have failed to see that they are using the same name for the same disease, viz. they would have recognized the identity of the English and French disease; as a matter of fact, they give us erroneously to understand that our infectious pneumo-enteritis is the French "rouget de porcs," a disease utterly different from it.

There are various assumptions as regards the biology of certain specific bacteria, for which not even an attempt at proof is undertaken. Thus the authors ascribe to the bacillus of diphtheria and the bacillus of typhoid fever the power to form spores, and this deduction they make from certain differentiated granules in the bacilli brought out by staining; but every bacteriologist knows very well that such proof is valueless, unless the alleged spores have been observed to be able to germinate; and unless certain experimental evidence (drying, heating, action of antiseptics) is forthcoming, no one would be justified in concluding that those granules in bacilli are spores.

Though the work, as has been remarked, is not all that could be wished, it nevertheless deserves to take a high place amongst the text-books on bacteriology. It is written in a very lucid style, and abounds in valuable and original observations.

OUR BOOK SHELF.

Verslag omtrent den Staat van 'S Lands Plantentuin te Buitenzorg en de daarbij behoorende Inrichtingen over het Jaar 1889 ("Report on the Condition of the State Gardens at Buitenzorg"). (Batavia: Government Printing Office, 1890.)

UNDER the able direction of Dr. Treub, the author of the Report before us, the botanic garden at Buitenzorg in Java has developed into one of the most important establishments of the kind in existence, and has become an active centre of both scientific and practical botany. The present Report renders an account of the staff, publications, library, herbarium, museum, botanical laboratory, chemical-pharmacological laboratory, botanical garden, experimental garden for trials of trees producing gutta-percha, together with copious lists of plants and seeds distributed and received. As usual during the past few years, a number of foreigners, chiefly Germans, have occupied tables in the laboratory, having travelled from Europe expressly for the purpose of availing themselves of the facilities there afforded for original researches. This Report is recommended to botanists contemplating work in the tropics. W. B. H.

The Electric Light. Fifth Edition. By A. Bromley Holmes, M.Inst.C.E. (London: Bemrose and Sons, 1890.)

QUITE recently we had occasion to refer to the rapid growth that has taken place during the last few years in the development and application of electricity, and at the same time we gave an illustration of one of the latest forms of gas-engines for its production. This work places before the reader (who is assumed to be quite ignorant of electrical science) a popular and intelligent account "of the means used for producing electric light." The author, at the commencement, conscious of the difficulty of employing technical terms, explains each in simple language, so that their meanings may be fairly grasped by the reader.

After showing how electricity can be produced by batteries, he explains how it may be produced by mechanical means, leading up to the latest forms of dynamos. A very meagre description is given of these dynamos, descriptions of which, if they had been more fully treated, would have added great interest to this part of the work.

Conversion of electricity into light, and storage of electricity, are the subjects of the next two chapters, in the former of which are described the various lamps employed in lighting, with an account of the many self-adjusting arrangements for the purpose of keeping the carbon poles at a constant distance from each other. The remaining chapters deal with the distribution and measurement of electricity, testing and necessary precautions, and selection of light, in which the selection of lamp and the arrangements for any particular purposes are discussed. The author also treats of motive power and cost, concerning the latter of which he gives some interesting statistics relating to the comparison of gas and electricity.

On the whole, the author has produced an interesting book, which explains in a simple manner the elements of electric lighting.

Maps and Map-Drawing. By William A. Elderton. "Macmillan's Geographical Series." (London: Macmillan and Co., 1890.)

THIS little book will be found to form a most useful supplement to the works on geography published in the same series. In it an excellent brief account is given concerning the history of maps from the records of the Egyptians down to the present day. Then follow various methods of making surveys, including descriptions of the various instruments employed, such as the prismatic compass

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theodolite, sextant, &c. In the section on the globes, a short summary is given of ancient and curious globes, succeeding which are descriptions showing how latitudes and longitudes, day and night, &c., are measured, also the principles of great circle sailing. Part iv. deals with map-drawing, in which a brief but plain description is given of the various methods of projection, such as orthographic, gnomonic, stereographic, conical, &c.; reference also is made to the different symbols used in map-drawing. The last two parts treat of map copying and memory-maps, of which the latter will be found of great importance, for, by using the method adopted, and carrying out the suggestions, the learner may remember much that might otherwise be forgotten.

Krystallographisch-chemische Tabellen. Von Dr. A. Fock. (Leipzig: Wilhelm Engelmann, 1890.)

THIS little work of ninety pages supplies a much-needed want in chemical crystallography. The increase of late years in the number of original memoirs, describing the crystallograpical characters of newly-discovered chemical compounds, and pointing out relations between many of the longer known ones, has been so great that the text-books now in use, such as that of Rammelsberg, are very much out of date. Dr. Fock presents the data acquired up to the present in a very condensed and easily referable form; a form, moreover, which at once exhibits such relationships as have been noticed between chemical composition and crystalline form. The tables will be found to include brief descriptions of all the more recent measurements contributed to the *Zeitschrift für Krystallographie*, as far as regards the crystalline system and the elements of the crystals measured. The arrangement adopted is somewhat similar to that in Groth's "Tabellarischer Uebersicht des Mineralien," but including in addition almost all the known chemical salts, and all the measurements of organic compounds yet made. The information furnished concerning each of the compounds mentioned consists of (1) its symmetry, (2) the ratio of its axes, (3) the axial angles of monoclinic and triclinic compounds, and (4) the observer by whom the measurements were made. In many places, where important relations have been noticed, additional information of a character extremely useful for lecture purposes is given. It is quite evident from the whole character of the work that great care and a large expenditure of time have been involved in its preparation, and those who are interested in the subject must feel greatly indebted to Dr. Fock for collecting such a valuable store of information in so handy a form.

A. E. TUTTON.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Mr. Wallace on Physiological Selection.

BY his second letter Mr. Wallace leaves no possibility of doubt touching (1) the manifest agreement, and (2) the alleged difference, between his recent theory of cross-infertility in relation to the origin of species, and the preceding theory on the same subject, as published by Mr. Catchpool, Mr. Gulick, and myself.

(1) The manifest agreement consists in supposing, as he says, that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species; and that such "incipient infertility" is of so much importance in this "process of differentiation" that its absence may be regarded as one of the *usual* causes of the failure of varieties to become developed into distinct species.

(2) The only point of difference alleged is, that while Mr. Wallace says this incipient infertility can *never* arise alone, or except in association with some other and preceding varietal difference, we (according to him) have represented that it must *always* arise "alone, in an otherwise undifferentiated species," and therefore always constitute the *initial* change in the way of varietal divergence.

Such being the only point of difference alleged, it is obvious in the first place that the allegation, even if valid, has reference to a point of but secondary importance. For if we are all agreed that the "incipient infertility," whenever it does arise, is a factor of such high importance in the origination of species as Mr. Wallace now admits, surely the question whether it can ever arise before (or can only arise after) incipient varietal characters of any other kind becomes a question of comparatively little consequence. But now, in the second place, the allegation is not valid, being, in fact, the very opposite of the truth. Taking first my own presentation of the theory, both in "the original paper and in the summary of it published in NATURE," I not only expressly stated, but carefully argued, that the incipient infertility may arise *either before or after* variations of any other kind; and, in order to emphasize this distinction, I devoted one part of the paper to the first class of cases, while relegating to another part my consideration of the second class. Therefore it is merely by an eclectic method of quotation that Mr. Wallace now represents that I began by setting forth only one side of "physiological selection," or the cases where incipient infertility is the prior change. Why he should persistently ignore all the other part of the same paper, or the cases where I show that incipient infertility need not be the prior change, I do not care to inquire. But at least the omission cannot be due to any want of clearness on my part, inasmuch as in his first criticism of the paper, which he published several years ago, he displayed a complete understanding of what I had said upon this point.[1]

After this much explanation it seems almost needless to say that I stand by every one of the "eight quotations" which Mr. Wallace has given. For "it [still] appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that the cases where it has been superinduced by the secondary distinctions are comparatively few in number;" "it is [still] on what may be called spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection;" I still continue to ask, "Why should we suppose that, unlike all other variations, it [*i.e.* the physiological variation] can never be independent?" And so on through all the eight selected sentences, provided that any regard at all be paid to their context and relation to other parts of the paper. For no one of these sentences in the smallest degree affects the position which from the first I have consistently and persistently held—viz. that it makes no difference to the theory in what proportional number of cases the physiological change has been the prior change. Indeed, the *immediate* context of the first of the above quotations sets forth that it would make no difference to the theory even if we were to suppose that in *no* case can the physiological change have been the prior change.[2] In other words, it is expressly stated that, even if we were to adopt the identical opinion on which alone Mr. Wallace now relies as constituting any difference at all between his theory and my own, still the latter, in its "principle" or "essence," would be in nowise affected. Yet Mr. Wallace now accuses me of "an absolute change of front" on the sole ground that I repeat these statements![3]

So much for my own paper. Mr. Catchpool's enunciation of the theory was much too brief to admit of any fair criticism of the kind which Mr. Wallace now passes upon it. But Mr. Gulick's elaborate essays—which he abstains from mentioning—are quite another matter; and, as stated in my last letter, they considered much more fully than mine had done the subordinate

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[1] *E.g.*, "Mr. Romanes then goes on to argue that, as a rule, these physiological variations are those which occur first, and form the starting-point of new species. He admits that in some ['possibly in many'] cases sterility may be a secondary character, due perhaps to the constitutional change indicated by the external variation; but even in that case physiological selection plays an equally important part, because if it [*i.e.* the incipient infertility] does not arise, either coincidently with the ordinary external variation, or as a consequence of it, then that variation will not be preserved, but will rapidly be extinguished by intercrossing with the parent form" (*Fortnightly Review*, 1886, p. 302). This brief extract is enough to show how widely Mr. Wallace's first representation of my "original paper" differs from his last, as regards the only point now in question.

[2] See NATURE abstract, vol. xxxiv. p. 339.

[3] There are several other distortions of my views in Mr. Wallace's letter, but space prevents me from dealing with them.

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